Neglected systems and theorizing: a comment on the transitions research agenda

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The new Sustainability Transitions Research Network agenda (Köhler et al., 2019) is an ambitious attempt to establish the frontiers of transitions studies. In this comment I propose two ways to broaden it in terms of thematic focus and the role of theory.

Dominant vs. neglected systems

Current transitions research predominantly focuses on energy, mobility and agro-food systems. This is well reflected by studies cited in the agenda. Given the contribution of these systems to global sustainability issues this thematic focus is understandable on one hand. On the other hand, the design of system interventions requires good transitions theory. And good transitions theory, in turn, requires the study of a diverse range of systems. These include systems that have not been studied as extensively to date as well as systems that have been virtually neglected.

Why? First, because we still do not know what we do not know. Attention to new systems, however obscure at first sight, might yield entirely new and unexpected insights that might be missed in the study of the “usual suspects”. For example, rock'n'roll has little to do with sustainability yet its study from the transitions angle (Geels, 2007) opened up an entirely new direction of multi-regime interaction, a topic of great importance for sustainability transitions (Rosenbloom, 2019). One could envision a research programme beginning with a systematic mapping of societal functions fulfilled by various socio-technical systems, e.g. energy, mobility, food, water, housing, waste, healthcare, communications, finance, defence, education or even reproduction. This would enable to find out what functions and systems have been relatively neglected. Case studies could then be designed to tackle them.

Second, systems with relatively low environmental impact may have important indirect effects. For example, an education system that would integrate sustainability issues to every subject on all levels could play a foundational role in supporting transitions in other systems by socializing a whole generation of people in a considerably different manner. One might also analyse the global demographic transition (Lee, 2003) as an interaction between education and reproduction systems that has resulted in a decreasing fertility rate. This, in turn, can contribute to the alleviation of resource pressures on energy, mobility and food systems. Understanding the internal dynamics of such neglected systems might thus turn out to be far more important than it would first seem.

Third, some neglected systems may be especially suitable for studying specific aspects of transitions. For example, the internet system offers an extreme case of user involvement: early users configured the system in a manner that made it very difficult

for economic and political actors to control it, leading to current clashes on network neutrality, privacy and surveillance (Abbate, 1999; Zuboff, 2019). This sequence of events, however, explicitly contradicts existing transitions theory which claims that system alignment precedes broader diffusion (Kanger and Schot, 2016). The internet case thereby highlights the need for a better theorization of links between actor involvement and system alignment. In sum: studying specific actors, factors and mechanisms in systems where they are particularly salient can lead to clearer theoretical formulations that, in turn, can be transferred to the study of energy, mobility and agro-food systems.

Theorizing transitions

Although the methodology section is a welcome addition to the agenda, more fundamental questions remain to be addressed by the community: how do we theorize about transitions and, in so doing, what mode of thinking do we prioritize? Taking stock of recent advances in sociology, management studies and political science I propose three ways to make use of a more reflexive approach to theorizing.

First, an emerging strand of literature has begun to identify different strategies for theorizing, e.g. using analogies, metaphors and functionalist explanations as a source of novel insights, zooming in on concepts to uncover their underlying "property space" (i.e. the analytical dimensions implied by the definition), making lists of preliminary and intuitively plausible causal factors, training counterfactual imagination, knowing when to engage in speculation and many more (see Abbott, 2004; Rueschmeyer, 2009; Swedberg 2014a, b, 2017, 2018). Awareness and systematic deployment of these strategies might help transitions scholars in future theory-building.

Second, I feel that presently transitions studies could make more productive use of a particular strategy: combining thinking on the empirically observable ("how things are?") with thinking on the logically possible ("how things should be on logical grounds?") (Taagepera, 2008; 2018). This is especially important given the recent emergence of systematic but largely literature-driven reviews in the field (e.g. Fischer and Newig, 2016; Kivimaa et al., 2019; Savaget et al., 2019; Sengers et al., 2019). Whereas such works can provide a good descriptive overview of existing findings they risk missing out on systematic gaps in the field. This problem can be alleviated, however, if empirical work is coupled with analytical reasoning about the types of categories that can be logically observed in the first place.

I will offer an example from my ongoing work to illustrate the usefulness of this strategy. Our team focused on the identification of intervention points: areas in the system or its environment where the application of policy instruments would facilitate transformative change. For that purpose we began from reflecting on existing transitions theory and what it implied about the likely loci of systems change, leading to the identification of six intervention points. Using these to code the literature on

policy mixes we discovered that the latter overwhelmingly focuses on only three of them. Therefore, we could make a credible claim that current literature had systematically missed out on some intervention points – points that could have been logically present in the studies reviewed but for one reason or another were not. Here the conceptualization on logical grounds provided us with a framework broader than the summary of existing empirical observations.

Third, more attention should be paid to the criteria by which new ideas are assessed and how these criteria might change over time. It has been argued that successful theories are both true and interesting, i.e. they challenge some (but not all) taken-for-granted assumptions of the target audience (Davis, 1971; Bartunek et al., 2006; Swedberg, 2016). However, as a field matures the number of shared assumptions of the community increases, making it easier for new studies to challenge "auxiliary" assumptions without necessarily going back to the core ones. This also creates a threat that truly boundary-pushing work that does challenge the core assumptions of the field will be increasingly rejected in the review process as too controversial. Furthermore, there are strong institutional drivers towards incremental science in every field, e.g. incentives provided by tenure systems and research assessment exercises to publish frequently in high-ranking journals, academic specialization, norms set by top journal editors and reviewers as well as active gate-keeping of self-interested networks of scholars (Grey, 2010). Over time the combination of these factors might drive a field in a situation of increasing membership but decreasing intellectual diversity (see Alvesson and Sandberg, 2013; Delbridge and Fiss, 2013, for insightful critiques of these tendencies in management studies).

In my view transitions studies are not there at the moment but the field cannot be considered immune to the threat of creeping incrementalism. Fortunately, we can learn from the experience of other disciplines. For example, in management studies explicit criteria have been developed to recognize the strengths of different theorizing styles, e.g. propositional, narrative and typological (Cornelissen, 2016). Methodologies have been created for assumption-challenging (rather than gap-spotting) research (Alvesson and Kärreman, 2011; Alvesson and Sandberg, 2011). One can also think of applying different assessment criteria for different types of works: whereas consensus between experts would be welcome for excellence-within-boundaries studies, conflicting views from established figures in the field could be taken as a sign of merit for a boundary-pushing one.

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